Chapter Five

Clay Boyce
Aerojet - AJ10-137 Apollo Service Module Engine

Clay Boyce left the U.S. Air Force in 1955, and joined Aerojet at its Azusa, California facility. After the launch of Russia's Sputnik Program, he joined the Thor/Able Second Stage Program, an adaptation of the Vanguard second stage. Subsequently, he participated in the conceptual design and development of the AbleStar upper stage. Variations of this stage are still flying. In 1960, he was assigned to the Win Apollo team and, then, became engineering manager of the Apollo Service Propulsion System (SPS) engine development. In 1969, he was assigned to NASA's Johnson Space Center in Texas for technical support of the SPS engine. Later assignments included the Space Shuttle Orbital Maneuvering System engine, the Japanese N-2 Upper Stage, and the National Aerospace Plane. Boyce retired from Aerojet in 1991, and has provided consulting services to the company since that time.

Some of the most tense times I ever had were on the Apollo mission. The question I always got asked the most when somebody talked about it was, "What if the service module engine doesn't start, especially when the astronauts are ready to come home?" I will tell you why it was tense. I knew it would start, but until that first mid-course firing on the way to the moon, I didn't know if they had connected it right. Once I got a firing signal, I knew they had mated the connectors properly in place.

Our Apollo proposal effort started for me in 1960. The Thor AbleStar vehicle was being developed then (See Slide 3, Appendix G). We used technology like the titanium nozzle for the

SPS engine. I was partying in a swimming pool down in Florida just after the second Able-Star launch when they came and got me about 3 a.m. and said, "We booked you a flight out of Orlando, 7 a.m. Be in Philadelphia, go to General Electric, find the Apollo group, and tell them our engine is the best." That was my first introduction to the word "Apollo" in the space program.

We had written twenty-nine proposals to twelve different primes (contractors competing for Apollo), and all but one of them selected our engine. We had a pretty good chance of winning the job. The engine was the same height as a space shuttle main engine, and the exit diameter was slightly larger than the space shuttle main engine (See Slide 5, Appendix G). The size was set before Apollo adopted the Lunar Excursion Module (LEM) mission approach. Prior to the LEM, the whole Command Module was to be landed on the moon, and this engine would have been the engine to lift it off the moon and return it to Earth. I would have had to wait a long time for that first firing, if they had followed the original concept. Aerojet was awarded the Apollo SPS engine development contract in April 1962, and the final decision on LEM was made about November or December of that year. We had proceeded far enough that even though they went to the LEM concept, they decided it wasn't worth starting over with a smaller, lower-thrust engine, so they kept the large size. The large size resulted because it was specified to be a pressure-fed engine. They didn't want to have to worry about the reliability of pumps and everything on the moon at that time. It was a 20,000-pound-force (lbf) thrust engine, but it was as big as a space shuttle main engine (SSME). Chamber pressure was only 100 pounds per square inch (psi). The major change that it made from the mockup was at the head-end, gimbaled configuration was changed to a throat-gimbaled design, which reduced the overall height of the Saturn vehicle when they put the LEM in behind the service module. The throat gimbaling of the engine saved about three and one-half to four feet of overall vehicle length and weight.

The technology we had at that time was the Saint/Apollo subscale engine (See Slide 4, Appendix G). It was for a satellite interceptor system for the Air Force. It used what we called earth-storable propellants, N_2O_4 (dinitrogen tetroxide, a powerful oxidizer) and some of the hydrazine families, because the Saint Program actually was an in-orbit satellite that was supposed to be able to detect an orbiting enemy satellite and go after it. That program was started in 1957-1958. It never flew because the computer capabilities and the control system it required just weren't mature enough to do it. But, we did build the ablative engine, the biggest at that time with 2,000 pounds of thrust. It had a nice titanium nozzle, an aluminum injector, and an ablative thrust chamber–all the things we needed for the SPS because we were looking for the simplest, lightest-weight engine we could get. We later used that engine for subscale testing of ablative materials and nozzle extensions for the full-scale engine.

The general configuration of the SPS engine was 20,000 pounds of thrust, with a chamber pressure of 100 psi and specific impulse (Isp) of 314.5. The very large nozzle had an area ratio of 62.5:1 (exit area to throat area). The propellants were nitrogen tetroxide (also known as N_2O_4 and nitrous oxide) and A-50. A-50 was a hydrazine family fuel. Aerojet developed it for the Titan Missile Program when they went with Titan II, to store it in the launch silos. They wanted the highest performance they could get. N_2H_4 was just pure hydrazine, which doesn't take low temperature very well. In fact, it freezes about like water. We started adding unsymmetricaldimethylhydrazine (UDMH) to the hydrazine until such time as it would meet the environmental specifications the Air Force needed for Titan II. It turned out it's roughly a fifty-fifty mix. We still had to be careful with that fuel because the two fluids didn't mix very well chemically. We had to spray the two fluids through some special nozzles to get them to emulsify with each other into a single fluid. If we ever got it too cold or froze it, the hydrazine separated back out. Then, if we tried to run the engine, things could go boom in the night.

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The inlet pressure was only 165 pounds per square inch absolute (psia), but we needed at least forty psi pressure drop across the injector just to get some kind of stable flow. It was a whole new game for some of us. We didn't have much supply pressure to work with. It had the aluminum injector to keep the weight down. That was a couple feet in diameter, and we didn't have a lot of propellant to cool it. In fact, we had to use both propellants to keep the injector cool. There were twenty-two ring channels in the injector. Specification required 750 seconds duration, or fifty engine restarts during a flight.

There were several first flight things we accomplished with the engine. It was the first ablative thrust chamber of any size to fly. (See Slide 6, Appendix G) There were no liners in it. It was just straight ablative material. It took us a while to figure that out. It was a throat-gimbaled engine, and it was the first engine to fly with columbium (also known as niobium, used as an alloying element in steels and superalloys) in the nozzle.

The first time we fired that assembly, we got a couple of surprises. (See Slide 7, Appendix G) The chamber pressure was supposed to be 100 psi. We fired it up, and it ran just fine for about a second, then the chamber pressure (Pc) dipped down to forty psi and gradually came back up to 100. The engine went on running, but we were holding our breath a little. One of our requirements was to use the fuel to actuate the propellant valve. With those low pressures, it pulled so much fuel from the inlet of the engine to actuate the valve that there wasn't enough to run the engine for a second or two. The good news was it stayed stable through all of that. During these very first firings, we hadn't received the requirement for dynamic stability, so the engine contained an unbaffled injector, and it recovered. It never really ever went unstable. It just turns out it was fairly throttleable.

There was another unexpected surprise that involved the interface with the actual Service Module. The decision for our approach evolved during proposal time. Half of the contractors looked at a single engine with redundant controls; others had groups of four and five engines for reliability. (See Slide 8, Appendix G) The final contractual decision, from a weight and an efficiency standpoint, was to make a single thrust chamber with dual redundant valving to assure that we could always get a start, and always get a shutdown. The gimbal actuators, which were the other moving parts on the engine, had semi-redundancy. The propellant valve was about two feet square and weighed 100 pounds, the biggest valve on which I ever worked. It was kind of a mirror image. One side of it was the oxidizer; the other side was the fuel. There were four ball valves in each propellant circuit in this arrangement: a fuel ball, a common shaft, and an oxidizer ball. The actuator to rotate the shaft was located between the balls. There were four of these individual assemblies utilized into a series-parallel configuration. On an engine start, any one of those actuators could fail, and the engine would still start. If, on shutdown, one of the actuators failed, it would still shutdown. We could run the engine, which we normally did with all four sets of valves open, but it would run equally well on one pair in each circuit, which we did a lot of in qualification testing.

One of the development challenges we faced didn't have to do so much with the valve operation as it did with manufacturing. It was a complex casting with lots of machining. We were having them machined outside our plant by a subcontractor, and it was taking him about a week to make one casting. Somewhere in the middle of trying to get more castings, Aerojet purchased what, at that time, was one of the newest numerically controlled machines with multi-axis operations. Management decided they needed something to do with it, so I got directed to bring the machining of the valve in-house. We did that, and it took a couple of months to get the things all set up. We started machining valves. On the fourth day of the six-day cycle, two shifts a day, the machine tool would jam a tool through a side of the casting and ruin it. At that time, the machine control was not digital. There was a punched paper tape, about one inch wide and over 100 feet long. Every time they would ruin one of those castings, they would have to go through that whole tape, make a few changes, and punch out an entire new tape. It was three and one-half months before we started getting good castings. After that, it worked fine, but it did set us back a little on our schedule.

The other major valve problem was the actuation. We were required to use engine fuel as the hydraulic actuator fluid. They wanted to minimize the number of connections that crossed the interface between the Service Module and the engine. We were only allowed the main propellant lines and one redundant, electrical cable. That was it. We switched to a pneumatic valve actuation because we had to stay in the same envelope, and we couldn't cross the interface. We removed the fuel hydraulic system from the cylinders and put a big spring in there that was used to close the valve and an engine-located, high-pressure nitrogen subsystem to pneumatically open the valve. We were required fifty starts of the engine. The amount of nitrogen we had would restart the engine about eighty times. (See Slide 8, Appendix G)

Those were the two major events during valve development. Once we got them going, we had the usual problems of seal material in the valve. We started out with Teflon, and it would cold-flow too much. We finally used some fiberglass-impregnated Teflon materials that would work. We had a three-micron finish on those valve balls, and they were each about two and one-half inches in diameter. It was a job manufacturing them, but not intolerable. The balls were made out of stainless steel. During one of our weight reduction exercises, we got the bright idea to make the balls from some lighter-weight material. We tried beryllium, which was about the only candidate that was significantly lighter. They worked, except that beryllium is pretty soft, and it didn't have wear life that would meet the mission requirement.

Our injector assembly was about two feet in diameter. It was all aluminum (See Slide 9, Appendix G). It had five baffles for the dynamic stability, and I think we did 240 or 250 bomb[1] tests in developing this baffle configuration. The original injector had twenty-two rings. There was one main rule in the fabrication of the injector: we had to keep parent material between the propellants. There couldn't be any weld that both propellants could touch. With the low pressures and the big surface area to cool, we didn't have much room left for lands between the rings. That was a problem. When we went to the baffles, we had to reduce the number of channels down to fifteen. When we started injector development, because of the narrow land area, we thought we would have a welding problem, so we decided we would braze the injector. Well, it turns out there were about 200 linear inches of area that had to be brazed. If we didn't have it perfectly cleaned and perfectly aligned,we didn't get a perfect braze. We never got a brazed injector that was 100 percent leak-free.

About that time, NASA was still looking at the future Nova launch vehicle, and Aerojet had the contract for the M-1 engine for the Nova's second stage. It was 1 million-plus-one-pound thrust hydrogen/oxygen engine. Aerojet had bought an electron beam welder for use on the program. When the M-1 got canceled, it sat in a corner, doing nothing. We decided we would give it a shot at welding the SPS injector, and it worked quite well. After we started firing the injectors for long periods of time, we encountered a problem that became a phenomenon in very low-pressure, large engines with the particular propellant combination we had. We named it "popping." We would start the engine up and we never knew when, but if we were watching the chamber pressure (Pc) trace on the oscillograph, all of a sudden, the trace would expand

[1] Here the 'bomb' refers to an impulse charge to induce instability in the combustion chamber for assessing the engine's susceptibility to combustion instability.

into a little football-like bubble. Then, it would go away. It would just last a few milliseconds. But, every time it would make a bubble, to us in the test bay, it sounded like a "pop;" hence, the name "popping." Eventually, it became a large-enough concern that NASA convened a meeting at North American Aviation, Inc., and brought all the stability experts from the universities, and we spent two days talking about that. I thought, "Boy, I'm finally going to get some help." The next day, I got the results. They told me they didn't like the name "popping." We eventually solved it. At the low pressures and the low pressure drop across the injector, the length of the drilled orifice became critical, and the propellant stream sometimes would come out clean, and sometimes would attach to the edge of the orifice, causing some propellant to flow onto the injector face. When the propellant stream ran along the injector face, it changed the combustion characteristics of the element, and it would cause the pop. It never hurt the engine, but the first few times really worried us. We counter-drilled those and got a shorter, fixed-length orifice that solved the problem.

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We had to do the bomb tests to check the dynamic stability. That turned out to be relatively easy to do. We had to worry more about damage to the injector from bomb fragments than from anything else. The ablative chamber had no kind of liners or anything inside; it consisted only of laid-up ablative material with some fluted aluminum flanges at each end (See Slide 10, Appendix G). At the time we started the program, and even back in the old Saint Program, we didn't have nice impregnated tape like they do today. The coated glass fiber string material was made in whatever length rolls you wanted. The continuous string was a little bit bigger around than a piece of heavy kite string. Whatever thickness chamber we wanted, we'd cut that length of strings and lay them on a piece of sticky tape. All of them were hand laid, side-by-side-by-side. The tape with the strings was then wound around the chamber mandrel (a spindle to support the piece during machining) and impregnated. We did that until the ablative material industry progressed and developed the flat tape. The flat tape made the chamber lighter because the ratio of the glass to resin was a lot greater, and provided more heat-dissipating capacity. This allowed the chamber wall to be thinner.

The next major component on the engine was the gimbal actuator. This particular actuator is a semi-redundant device. I say "semi" because the main actuator movement device was a single ball screw, much like that used in present-day car steering mechanisms. The ball screw was operated by two electric motors transmitting power through magnetic particle clutches. One motor/clutch assembly extended the actuator, and one retracted the actuator. There was a completely redundant set of motors and clutches in each actuator for mission reliability. During engine operation, all four motor/clutch assemblies were running, and control signals from the spacecraft activated a specific magnetic clutch for the desired engine position. A couple of our problems with the actuator didn't have anything to do with technical accomplishment. At the time we got the contract for the SPS engine, we planned to subcontract the actuator to a company called Lear Inc. in Grand Rapids, Michigan. Lear was the biggest actuator company in the U.S. at that time; all Boeing and McDonnell Douglas Aircraft Company airplanes used Lear actuators for moving controls surfaces and flaps. They had the best reputation for that. We gave them a contract, and they went to work.

The actuator assembly had to be enclosed in a pressurized, hermetically sealed can. (See Slide 11, Appendix G). The electric motors and the magnetic clutches wouldn't work in the vacuum of space. After a year or so, we received a pair of actuators to start testing. Both units were overweight and larger than the required envelope. Right about that time, another company by the name of Siegler bought Lear. The Lear group kept working the size and weight condition, along with a stiffness problem. Then, Siegler bought another company called Jack and Heintz Motor in Cleveland. Well, the Lear plant was overloaded with work; they didn't want to build a new building, and the Jack and Heintz plant in Cleveland had lots of empty space. Lear/Siegler moved the actuator program to Cleveland. This was a problem because the actuator engineers wouldn't move to Cleveland. All of a sudden, we had this contract with about half the money spent, but we didn't have a part, and we didn't have an engineering staff anymore. About that time, North American Aviation and NASA were getting nervous enough that they decided we'd better find another actuator company. Meanwhile, a couple of people who had worked at Lear had gone to work for an actuator company down in Costa Mesa, California, called Cadillac Gage. They were building actuators for the nozzles on the Polaris missile under contract from Aerojet, and had produced good hardware. The ex-Lear people talked to the Cadillac Gage management, and they gave us a bid to build the actuator. At that point, NASA and North American interceded and said, "We have already spent half the money. We can't afford that again. You are directed to give a fixed-price contract." We gave them a fixed-price contract, and they started to work.

Then, the second problem we found on the Lear actuators resurfaced. The stiffness requirement for the actuator was to be a spring rate of 300,000 inch-pounds to meet the vehicle control system loop frequency. The stiffness on the two actuators we'd received from the old Lear Company and the first two from Cadillac Gage were well below requirements. One of them was about 220,000 inch-pounds; the other one was 160,000. By then, the second company had used up all their fixed-price money and a little bit more. They said, "Sorry, but we have to stop work." It became a contractual battle for a while. It eventually was resolved. About that time, back at NASA Headquarters in Washington, D.C., Joe Shea, then the chief engineer of the Apollo Program, was reviewing his overall Program Evaluation and Review Technique (PERT) chart, and the actuator popped up as the umbrella for the whole Apollo Program, because if we didn't have that, the vehicles didn't go. Shea came to visit us one day. We went through everything with him. The North American guys were there and everything. After the review, they all went back to North American and said they would call us with their recommendations.

The next afternoon I got a call that said, "Don't bother to come down. The problem is solved." I said, "What happened?" My 300,000-pound actuator was being tied to a 90,000 inch-pound spring rate bracket on the Service Module. It didn't make any difference as long as my spring rate was over 100. By then, we'd worked for almost two and one-half years trying to get that thing stiff, so we hadn't performed life testing on the motors and magnetic particle clutches. The clutches became the next challenge. There had never been a magnetic clutch that big. Existing technology were little things, less than three-quarter inches in diameter. This one was not quite three inches in diameter. As you might expect, we found we had overheating and other problems. We got it all done, but the problem illustrates the fact that sometimes you run into events that have nothing to do with your engineering capability. My final challenge out of the actuator development was the boss saying, "Okay, go get out of that fixed-price contract." If you've read government contracting regulations, you know there's no way you could do that. I had to figure out a whole different contracting scheme so we could get Cadillac Gage back to work. (See Slide 11, Appendix G.)

We were scratching our heads about what to do when one of those fortuitous events occurred.

The first nozzle for the Service Propulsion System engine was developed out of Saint and AbleStar technology. The nozzle was made out of titanium. We calculated that the area ratio at the head end was about 6:1 as the point where exit temperatures would be down to titanium capabilities for radiation cooling. The exit area ratio was 62:1. The first problem came with fabrication of the gores. It takes sixteen gores to make one nozzle. Our nozzle fabrication contractor's plan was to hot stretch-form the titanium, make a mold of the shape we needed, and use Calrod® units (elongated heating elements) to make it hot. That, in turn, would heat the titanium stretched over it. That didn't work too well. We were using titanium for the nozzle because it took lots of temperature. We had to get it very hot to stretch-form it. Titanium has a tremendous memory. It doesn't want to stay in a new place. It kept wanting to go back to its original flat shape when it cooled. We were in a quandary about that. We had only been able to make four gores that were even halfway close. We gave them to our subcontractor who was developing the technique for welding them all together. We were scratching our heads about what to do when one of those fortuitous events occurred.

About the same time we started scratching our heads, the then-president of our company was at a cocktail party in Washington, D.C., and he got to talking with the good lady senator from Maine. She said, "Dan, I gave you a bunch of money, supported you for that Apollo thing, but you're not putting any of that work in my state." I then got a memo that said, "Go see if they do anything in Maine that'll help your engine program along." I put all my problems in my briefcase and went and found a little guy in Maine who worried about the state's economic development. He listened to me for about half a day and said, "We've got nothing like that up here. But, two weeks ago, there was a guy in here wanting us to help him start a small fishing boat business. He wants to make them out of aluminum, and they're all nice and round, instead of square. He had a machine that makes stuff kind of round and long like you're describing." I got the boat maker's name and an address, which was down in Massachusetts.

When I got to the address, it was at a road crossing with a little building about the size of an old outhouse sitting there and that was it. Finally, about a mile away, I found an old cotton mill from back in the textile days, and here was this little, old Norwegian toolmaker, and he had built this big machine. He was making boat parts, specifically the double-curved part of the bow. Well, he hadn't been able to sell the boat business, so he had taken a couple of contracts to make gores for radar antennas.

Those gores, curved two ways, weren't too different from what was needed for the nozzle. I described what I needed and he said, "Yeah, I think I can do that. Send me a mold and send me some material." We did. Probably a month or six weeks later, he called and said, "Come on back. I'll show you what's going on." We went back to his shop in Massachusetts, and he had been working with his machine, and he had made a bunch of gores out of aluminum while he was working. So, I was going to be there for the first titanium pull. When we had tried the hot stretch-forming, we had to work with one big sheet of titanium. The boat maker's new process worked so efficiently, he could cut the sheet diagonally and we could get two gores out of every sheet. He laid the sheet out on his machine, brushed on a lubricant from a nearby pot sitting on a hot plate, and started the machine. It was a cold-working process, and the machine put that titanium through something like old washing machine washer rollers. He could vary the axis of the rollers and their distance apart. The machine put that titanium through a little S curve about two inches high; it took away all its old form memory and put in a new one.

The very first one that rolled out the end of that machine looked perfect to me, better than any I had seen. We laid it on the mold, and he got a little hammer and went all over it. "Well, it's not right. There's one little place that sounds a little different," he said. He went over and tweaked his machine, put the same part back in there, ran it through again and had a perfect part. I said, "How many of those could you make a day?" He replied, "I can probably make twenty of them in a day." I said, "How much?" He said, "Fifty bucks apiece, if you supply the metal." That was the last gore I got from him for three and one-half months because his business didn't exactly meet NASA 200-2 quality specifications. I had to create a Quality Department, a Purchasing Department, all of the "isms" and documentation to certify the parts were useable for nozzle fabrication. But, by then, that problem was solved, so I didn't worry about it anymore.

We finally got a nozzle together, and because of the size and the pressure, the only place that you could hot-fire test it was in the big Air Force altitude facility at Tullahoma, Tennessee. I talked recently to a gentleman who works at Arnold Engineering Development Center, and we decided the tests were conducted at the facility's J-3 cell, a very tall structure, that was used. I found out later when we got the test cell operating satisfactorily, that firing the SPS engine caused the test stand to flex and caused misalignment of the performance measuring instrumentation. The thrust and side forces measuring cells had to be biased for the thrust stand movement so we could get accurate test data. Around early 1964, we got the first altitude engine all together and set up at Tullahoma. For the first engine firing, I was sitting in the control room with one of the North American Aviation guys. We heard the big countdown going on, and they finally said, "Fire." We were looking at our oscilloscope and nothing seemed to be happening. We thought, "Man, that engine runs awfully smooth." Then, somebody said, "Okay, fire that engine," and they threw the SPS start switch. It turned out the whole countdown was for the rocket engine injector they were developing to pull the tunnel down to altitude. *That* was the big thing to them. We were going to fire the SPS engine for fifty seconds as a checkout. We noticed a little decay in thrust level after ten to fifteen seconds. But, then, it leveled right off and everything was fine, and we thought we had a successful run because everything looked very good.

As expected, the nozzle got very hot. Things finally cooled down, and we were able to get in the cell. It had gotten hot all right – too hot. The whole nozzle had moved forward about eighteen inches. About ten inches down, the nozzle had curled right back over itself. We had a nice, big S curve all around and a nozzle that was a foot and one-half shorter. That's when we figured out we needed columbium. The only reason we found columbium is when titanium wouldn't work, we went to the periodic table of the chemistry book and said, "Okay, what's the next lightest metal?" We determined it needed to handle at least 2,100 degrees Farenheit. We were running around 1,900 degrees at the nozzle attach point, and columbium appeared to be capable. Then, we tried to find some columbium. It turns out the nuclear power industry used columbium to make the rods in which they put the nuclear material for power plants. We went to the company that was making those rods.

The company was called Wah Chang, in Albany, Oregon, and was owned by a very nice Chinese gentleman. We wanted thin material. All the material he made was one-half inch or more plates. He thought he could make us some thinner stuff. We wanted it forty-thousandths (0.040)of an inch thick. He was able to produce the thinner sheet stock we required. The titanium nozzle had a machine flange on the front. We tried to follow the same design. We were going to make a flange out of columbium also, but nobody had ever made anything like that out of columbium. They had always used the flat roll stock. But we found a company that would try to make a casting. They had a big, spin-casting rig. That flange was around twenty inches in diameter. After making a casting, it turned out they never had machined columbium, and it is extremely tough to machine. They could get about two cuts around the flange with one, good silicon carbide tool, then it was shot. We later got rid of the flange and figured out an attachment where we could just roll a lip on the end of the nozzle sheet material and clamp it to the end of the ablative chamber. While they were busy making sheet metal stock for the full-scale nozzle assemblies, we got a small piece of columbium and just rolled it up to make a small regular conical nozzle. We put that on the Apollo subscale engine and took it down to Tullahoma to see how the columbium worked. We fired a partial simulated SPS duty cycle. That was 200 seconds firing and one half-hour coast, then re-fire. Everything went fine until the re-fire, when the thrust level was not where it should have been. We opened up the tunnel, and the nozzle was gone about two inches down from the flange. It was just all jagged.

We determined that the temperatures to which we raised the columbium caused it to absorb hydrogen from what little atmosphere there was in test cell. The hydrogen crystallized the columbium. The inside of the nozzle, next to the fire, was clean and nice–no penetration there. But, on the outside, you could see a crystalline surface. The engine re-starting shock had shattered the nozzle.

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Operating the engine out in space would have been fine, no hydrogen. But, we had to develop and qualify the engine here on the ground. We had to find some way to protect the columbium so it could pass all the tests. We went through about twenty different coating materials and finally found one that North American Aircraft Division was using in some of the jet engines exit areas. We still had the same problem. We had to get the columbium up to 1,800 degrees to bake the coating on. There were ovens big enough to get up to the temperature we needed, but none of them had vacuum capability. Finally, working with North American Aviation, we developed a big retort (a closed laboratory vessel) that we could set the nozzle in, weld it all closed, pressure-test it, put in an inert atmosphere, and, then, put that whole business in the big oven. Remember, that nozzle was about ten feet tall and about eight feet in diameter. Facilities with the capability to heat and bake the large assembly for several hours were not readily available. Final nozzle configuration used columbium down to about the 40:1 area ratio, then titanium the rest of the way to the end. There were some quite unique issues in developing the welds between columbium and titanium. We welded them together using titanium rod. Micrographs of weld samples looked like there was no bond between the columbium and the titanium. Columbium material surface appeared just as smooth as it was before welding. But whenever we performed pull tests of joint samples, failure occurred in the weld-heat affected zone of the titanium. Whatever went on in there was strong. It worked well.

The SPS engine flew nineteen times. (See Slide 13, Appendix G). The most starts probably were performed on three of the engines, eight starts per engine, with 6,000 seconds of total time. In the overall development program, I know we tested more than 200 injectors. I think we had about 230 bomb tests. We went through 124 different injector patterns to get one that was totally compatible with the ablative material without any liners, and still approached the 98 percent combustion efficiency we needed without making any grooves down the ablative chamber. All in all, the total testing during the development and qualification totaled about twenty-eight hours on the engine assembly. That's a long time for that small engine.

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Just before the launch of the Apollo 8 mission, which took the first men out around the moon, Tom Paine was the acting administrator of NASA. He called George Mueller, who was the associate administrator for manned space flight. He said, "George, are you sure that engine is going to start? Find out." Mueller sent a team off to study the engine. The results of his study were the reason I knew the engine would start. He said, "There have been over 3,200 starts of that engine in development and qualification. There were only four times it didn't start, and those were all due to test stand stuff." The engine had started every time all through the development and qualification program. When I used to go to my little hometown in north Idaho, which was a long way from the space program, some of guys back there would ask me what I was working on. I told them, and they said, "What if it doesn't start?" I said, "It'll start. I'll lay it out for you. You probably start your car an average of three times a day. In a year, you've started it a thousand times. I'll bet you at least once, it doesn't go *hmmmm*, and, then, it's going to grind a little. That's one failure in a thousand." The specification I was working with was one failure in 5 million starts. So, I knew it was going to work as long as North American Aviation put that electrical cable on correctly.

Editor's Note: *The following information reflects a question-and-answer session held after Boyce's presentation.*

QUESTION: You had your fuel and your oxidizer in one valve assembly; would you do that differently now or did that work well enough?

BOYCE: It worked very well because the two independent castings had a cavity in between them. We had dual seals on each shaft where they went into the cavity/actuating area, and that area was vented. There never was a problem there. The biggest difficulty they had with respect to propellants was decontamination between the series ball valves after engine testing. There is a similar arrangement on Aerojet's Orbital Maneuvering System engine on the space shuttle. There is not a bi-propellant issue. Between the two series valves, there's always a little propellant trapped on landing, and they have to decontaminate those areas before returning to the hangar. Obviously, we didn't have that situation after an SPS engine flight. Nobody got that near to it.

QUESTION: Out of those 6,400 seconds, what percentage would you say was development versus qualification?

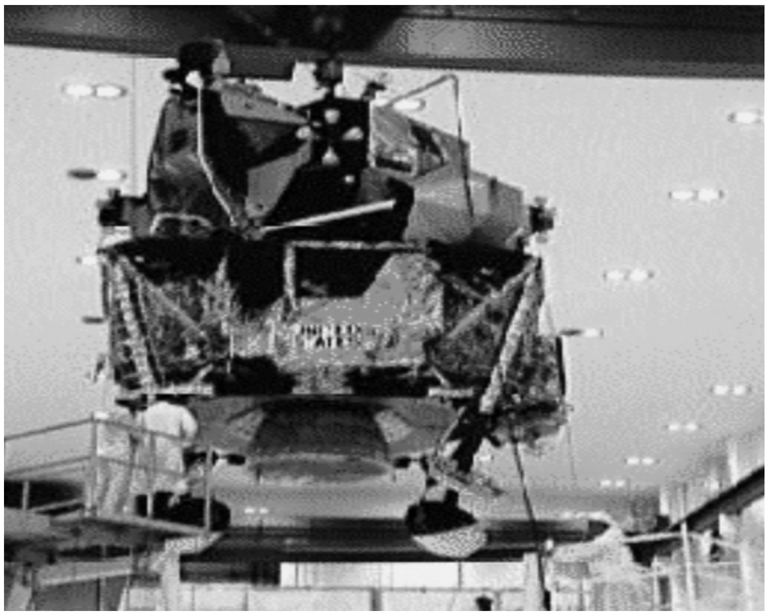
BOYCE: I would say probably 50 percent to 60 percent of those seconds were logged in development prior to starting qualification. There were some additional development tests in parallel with qualification testing to evaluate issues that arose during qualification. Here's a little summary of development and qualification: 216 injectors in the program, 3,200 firings, and 700 minutes of time on injectors; nozzle extensions, thirty-one units made, 1,400 firings for a total of 700 minutes. We made 274 combustion chambers, with 1,400 firings on the thrust chamber, and about 900 minutes of test time. We had twenty-seven total engine assemblies and 4,000 firings of the all-up engine assembly, which totaled about 1,700 minutes.

QUESTION: Early on in the design concept, did you have abort requirements already factored in or did those come later? That is, could you use the engine as an abort?

BOYCE: I think in one of the first Earth orbit manned launches, there was an early shutdown problem with one of the Saturn stages; they fired the SPS engine to put the Command/Service Module into orbit. It wasn't planned to be that way. During Earth orbit missions, the SPS was fired for engine operating characteristics tests, orbit corrections, and de-orbit. Remember, the original concept was that it would never fire until the astronauts were ready to come home from the moon. It did, of course, take over midcourse corrections; lunar orbit insertion and Earth return orbit insertion after they went to the LEM concept. After adopting LEM, there were no specification changes that changed the engine capability of fifty starts and 750 seconds total firing time. They did change the mission duty cycle firing schedule to match the LEM scenario. During the qualification program, an "abort" firing of 610 seconds was demonstrated. Worst-case mission requirements were thirty-six starts and 546 seconds of burn time - well within original specification.

STEVE FISHER[2]: So, apparently it wasn't part of the requirement, but the capability was there in terms of numbers of seconds.

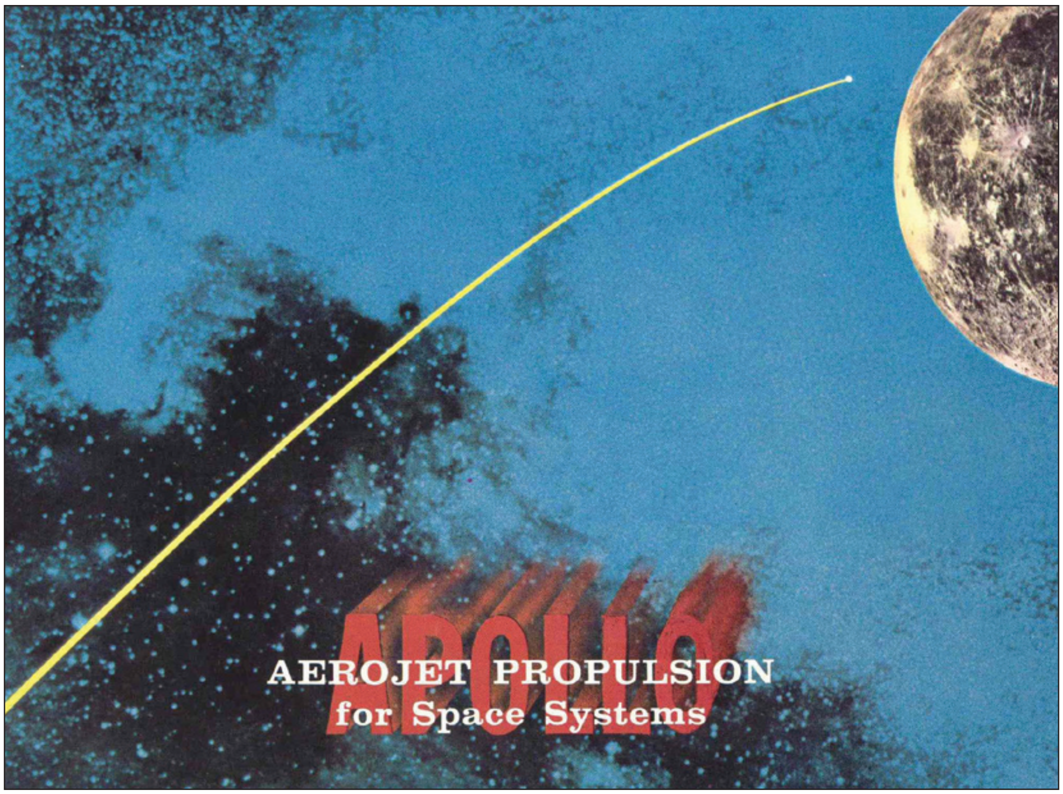
2 Steve Fisher served as facilitator during the *On the Shoulders of Giants* seminar series.



Lunar Module Descent Engine

Appendix G

Clay Boyce's Presentation Viewgraphs



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BRIEF CHRONOLOGY OF EVENTS DURING DEVELOPMENT OF THE

APOLLO SERVICE MODULE ROCKET ENGINE

Clay Boyce

Aerojet (Retired)

Sacramento, CA

Presented at

Stennis Space Center

April 25, 2006

APOLLO SPS ENGINE

AEROJET

- Proposal Task Start
  July 1960
- Contract Start
  April 1962
- Pre LEM Decision
  Concept

APOLLO SPS ENGINE

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- Technology Base
  - Saint Program
  - Apollo Subscale

APOLLO SPS ENGINE

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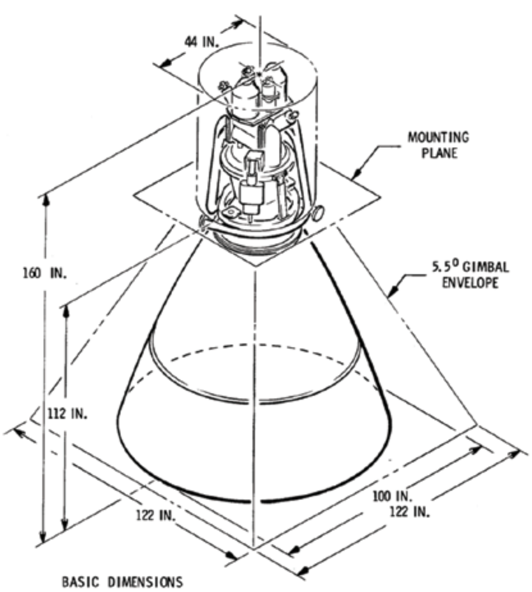

Thrust	20,000 lb
Propellants	NTO/A-50
Chamber Press	100 psia
Inlet Press	165 psia
Exp Ratio	62.5 to 1
Duration	750 sec
Restarts	50

APOLLO SPS ENGINE

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- Flight Firsts
  - Ablative Thrust Chamber
  - Throat Gimbal
  - Columbium Nozzle

APOLLO SPS ENGINE

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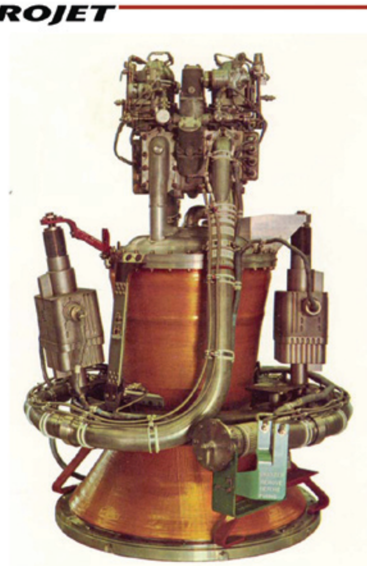

- **Final Powerhead Assembly**
  - 1st Firing Surprise
  - SPS Module Interface Surprise

APOLLO SPS ENGINE

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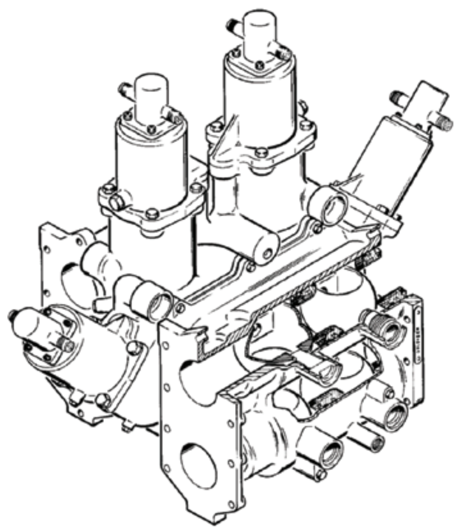

- Propellant Valve Assembly
  - NC Machining
  - Actuation

APOLLO SPS ENGINE

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- Injector Assembly
  - Aluminum Brazing
  - Popping
  - Dynamic Stability

APOLLO SPS ENGINE

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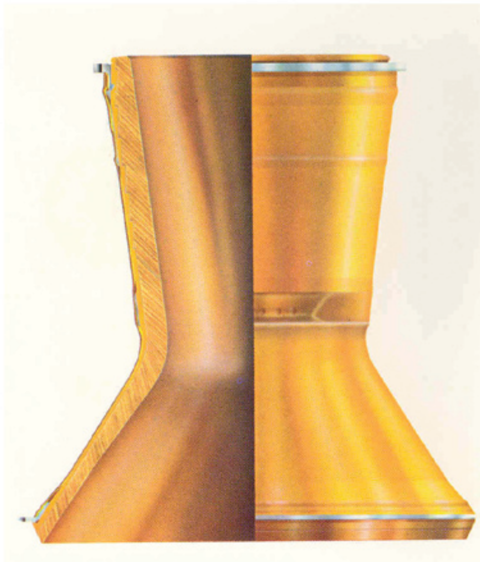

- Thrust Chamber Assembly
  - Ablative Material
  - Flanges

APOLLO SPS ENGINE

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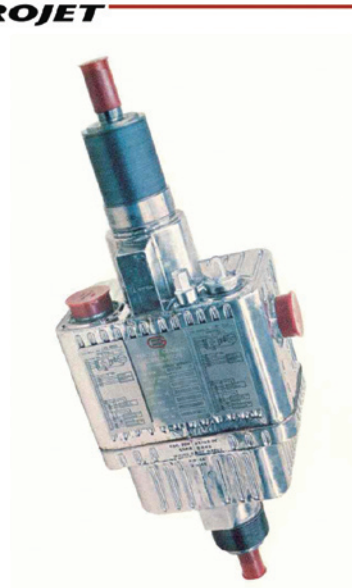

- Gimbal Actuator Assembly
  - Contract Events
  - Design Events

APOLLO SPS ENGINE

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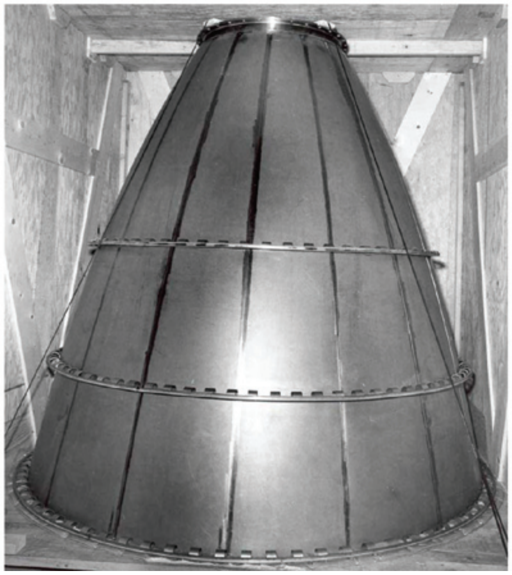

- Nozzle Assembly
  - Gore Fabrication
  - 1st Hot Fire
  - Columbium

APOLLO SPS ENGINE

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- **Firing History**

	No. Missions	No. Starts	Total Firing Time (sec)
Apollo Unmanned	**4**	**9**	**1,232**
Apollo Manned	**11**	**64**	**5,060**
Skylab Manned	**3**	**20**	**115**
Apollo/Soyuz	**1**	**9**	**18**
Total	**19**	**102**	**6,425**

First SPS flight was February 26, 1966
Longest single firing on a mission was 445 sec. (Apollo 6, Apr. 4, 1968)
Shortest firing on a mission was 0.5 sec. (Apollo 7, Oct. 11, 1968)
Most firings for an engine on a mission was 8 (Apollo 7, 9, and 15)


AEROJET
aerojet review
July 1979
A publication of Aerojet-General Corporation
Yep, now let's fire up this Aerojet SPS engine and head for home…
Wow, can it be that 38 years have gone by?
Man's first voyage to the moon– it really happened 35 years ago